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RESEARCH INSTITUTES AND THEIR VALUE[1]

IN this restless, drifting world in which we now live, even intelligent people are not always appreciative of the fact that many if not most of the great intellectual achievements in various fields have been accomplished only when the thinker has been protected from the interruption and annoyance of passing events and permitted to work out his ideas somewhat apart from the general current of existence. In the Middle Ages, the alchemist, the philosopher or the mathematician retired to a garret or cellar and there achieved his purpose, and even to this day the idea that starvation and a garret are successful stimulants to scientific investigation clings persistently to the popular mind, together with so many of those superstitions by which humanity is still largely guided. Truth is that the thinking man in the middle ages was driven into a garret and often compelled to accept poverty because his thoughts or discoveries had no commercial value or popular interest, and, if published, sometimes led to controversies settled once for all by that unanswerable argument of authority, the fagot and the stake. The example of Servetus must surely have been a severe blow to hasty publication. One of the early masters of medicine, he died a martyr to his printed opinions at the early age of 42, his old friend, John Calvin, seeing to it, it is said, that the fire was well started.

But the time when important extensions of the boundaries of knowledge, especially in science, can be accomplished in garret or cellar with no material except brains, a little sealing wax, some wire and a few pieces of glass,

[1] An address delivered at the opening of the new laboratory building of the Collis P. Huntington Memorial Hospital, Harvard University, May 15, 1922.

which was about the equipment with which Faraday made some of his most valuable discoveries in electricity, has long since passed. Brains are still the chief essential, but modern science has gone in most of its phases beyond the stage of easy discovery of important principles. No clearer demonstration of the fallacy of the popular belief in the capacity of the man in the street to solve complex problems exists than the report of the Naval Consultation Board in which it is shown that of one hundred and ten thousand suggestions received only one in a thousand were even worth considering, and of this one hundred and ten only one was put into production. A few highly trained scientific men, on the other hand, made most of the useful discoveries. To-day scientific advance in most fields depends upon the use of equipment of great delicacy and precision, and unfortunately only too often of very high cost. The time calls, therefore, for the organization and classification of research problems and a higher degree of collaboration between scientists than has ever been had before, and it is characteristic of that vision which has so often been a quality of Harvard thought and action that we are gathered together to celebrate the opening of a laboratory devoted to investigation in a field of science but newly set aside, that of biophysics. The name is new, though the science itself is not. When the professor of anatomy in the University of Bologna first used frogs' legs as a galvanometer to reveal the presence of electric currents, he was studying biophysics, even if in a somewhat elementary form. In our own times this new field for research has been sequestered from the disciplines of biology and physics as a special region, possibly because the knowledge of the chemistry and the physics of the human body has reached a point in its advance at which there is a little slowing-up in the rate of important discovery. In such a dilemma a shrewd scientist does not keep up a frontal attack, but quickly shifts to a slightly different approach to the problem. Thus, by the combination of the technical methods of physics and of chemistry in the study of living matter there is promise of an ample yield of valuable knowledge within the next few years and of a material advance which may possibly again illuminate the purely physical and chemical methods of attack on the secrets of life and in consequence lead to still further achievements in those fundamental sciences. Illustrations of the fertilizing value of this method of shifting the line of approach can be culled from the lives of many successful investigators. Pasteur is said to have started early in his life on the study of tuberculosis, but to have dropped it quickly when he found that he could make no headway with the technique then in use. If he had persisted, his name would not be known to-day. Paul Ehrlich spent several years investigating the problem of cancer, but as soon as he found that progress was slow and far-reaching results were doubtful, he quickly shifted to the more profitable field of an attack on parasitic diseases by means of chemical compounds, and there achieved a great and deserved success.

As it is one of the marks of genius to overcome obstacles with the least possible waste of energy, so the fact that this special field of biophysics has been selected for a concentrated attack affords an admirable criterion for the intelligence of those controlling the funds for cancer research in Boston. The world will profit by the investigations which in the future will be made in this laboratory, for in contrast to the worker of the older days, who so often concealed the results of his studies in order that he might reap some benefit from them, the modern scientist gives freely and at once to the public everything he achieves. He does not conceal or patent a valuable discovery which would in any way relieve human suffering.

The true investigator's chief stimulus is the love for his science and ambition for his institute; and the responsibility imposed by the great opportunities at his disposal will be, if he is the right sort, one of the strongest forces in sustaining the arduous labor of research. This concentration of responsibility and the development of intellectual power and leadership as problem after problem is solved is an important factor in the success of a truly scientific institute, a factor the psychology of which has often been overlooked by those administrators who wish to impose the regulations of the machine shop in order to obtain quantity production in science.

Besides the direct way in which an institution like this, devoted to research in some phase of pure or applied science, benefits humanity, there is also an indirect influence, not so fully appreciated. This is the reflex effect upon the university as a whole, for only by the possession of such centers of intellectual concentration does the university become a university in fact rather than in name. Every great teaching institution should be surrounded by a constellation of independent institutes such as this, devoted to the amassing of pure knowledge, without a view necessarily to its future use or practicality and without the encumbrances to effective thought which go with administrative work of the teaching of large numbers of immature students. Our men of genius in the universities still do too much undergraduate instruction and teach the teachers too little. This is one of the great defects of the present scheme of education, in that it accentuates routine and overlooks the spirit. When a university possesses a genius he should be tenderly protected and cherished. The ragweed will outgrow the orchid, as has been proved a thousand times. Why sacrifice another orchid to the test? But in research institutions lies true freedom of thought in the university. While to the undergraduates we must temper somewhat the boldness of our theories, in the research laboratory everything must be free. No one can foresee in what direction investigation must proceed. No hampering politicians, as in some state institutions, should be allowed to control the direction and type of investigation to be done, their equipment for this function as regards the natural sciences being usually somewhat less than that possessed by our Great Commoner, who is making so brave and useless a fight against the dangerous theories of evolution.

Who in his wildest moments could have imagined that the classification and anatomical study of the fleas which infest lower animals could ever have been of use in the saving of human lives? Yet when the Oriental plague threatened this country, in the results of such studies was found the means of combatting the disease, the uncontrolled ravages of which can best be learned by a reading of that old classic of Daniel Defoe's, "A Journal of the Plague Year." When we realize that because of our knowledge of public health obtained by research on apparently unimportant matters the repetition of such a plague is now impossible, we must be grateful to some of those who have made heavy sacrifices in the cause of science.

A few institutions like this will answer most effectively the statement recently made in the daily press that the foot-ball coaches had done more for Harvard than all the professors would ever accomplish—and this of a university which can claim Agassiz, Lowell, Norton, Child, Gibbs, Shaler, Royce and William James as only a few among those who have passed on. To enumerate the names of the living who are still doing for Harvard what these men did would be an insult to the intelligence of my audience.

The new building which we are gathered to inspect shows in its very architecture the thoughtfulness of those who planned it—simple as every workshop should be, for that is all a laboratory is, a place for labor. It shows that the money which has been given has gone on the inside rather than on decoration. I look forward to a day when architects will sacrifice all their art for the practical in laboratory building, and reserve the demonstration of their skill for libraries, museums and other structures which may properly give room for the display of artistic qualities.

But the building is not important. An institution of this type is always, it has been well said, the lengthened shadow of a great man. Those who are to work in it are far more important than any physical structure. The name in itself gives promise of long and useful service, bearing as it will the title of a line of famous surgeons. The annual reports of the Harvard Cancer Commission show how much has already been achieved. There are few groups of investigators in any country who have produced with relatively small means so much of sane, cautious, solid research work in cancer, biology and physics as have those who in the past have worked in the Huntington, and who are now to enjoy greater facilities, and so may properly be expected to do more and more as the laboratory expands. For

expand it inevitably will. It is said that opportunity knocks but once at the door, but this is the opportunity of receivers, not of givers. To the latter there is no limit. If this building had been built and equipped five years ago, we might not have had to share with our great scientific rival on the continent the discovery of many capital facts concerning the X-ray, for it was only the lack of equipment which kept the brilliant group of physicists who, under the leadership of Professor Duane, have made so many important advances in the theoretical study of X-rays, from covering many of the practical phases developed instead by our continental colleagues. The verification of the quantum relationship between the frequency of the X-ray and the voltage applied to the tube, as demonstrated by Duane, Hull and Webster, is a shining achievement which might easily satisfy any university for a long period of time. The work of Tyzzer on animal tumors especially laid the foundation for much recent research, while the demonstration by Bovie of the relationship between certain light rays and the coagulation of protein and the killing of cells is also a most important contribution to the newer aspects of biophysics. Whether the problem of cancer—that last great and as yet unanswered question in medicine—will be solved here, no one can say. But I am sure that the attack will be a brave one and that the results will be characterized by the same scientific caution and freedom from attempt at dramatic effect that have marked the work of the Harvard Cancer Commission in the past. We all look to this laboratory as the source of the highest type of scientific investigation combined with an unusual amount of common sense on the human side, due obviously to the influence of the director, Dr. Greenough. There is no reason to think that with the passing of time there will be any change in this high standard.

Let us all hope then that this building and its equipment and staff represent merely a beginning from which research will go forward on a broader and broader scale, until at some future time we may have a better insight than at present into what has hitherto successfully evaded human inquiry—the nature of life and growth. When that goal is achieved the solution of the cancer problem will be in sight.

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THE EFFECT OF THE NATURE OF THE DIET ON THE DIGESTIBILITY OF BUTTER

IT is estimated that in the United States about 18 pounds of dairy butter are consumed per capita yearly and of this amount the larger portion is used for table purposes. This indicates quite conclusively that in spite of the increasing variety of fats available for table and culinary purposes, dairy butter still remains one of the most popular and widely used edible fats. Formerly it was very generally believed that the principal if not the entire food value of butter was due to the energy which it supplied to the diet. The recent discovery that dairy butter contains a relatively large amount of vitamin A, which has been shown to be essential for an adequate diet, has served to further increase the popularity of this extensively used fat.

The very general use of butter for food purposes is no doubt responsible for the early and continued attention that has been given to a study of its nutritive value by physiological chemists and nutrition experts. Many digestion experiments have been carried on both in this country and in Europe to determine its digestibility, but since the experimental procedures of the different investigators were not uniform the results obtained do not permit of direct comparison. The lack of uniformity in experimental conditions is perhaps most noticeable in the wide variation of the nature of the basal ration used by the different investigators. However, this variation in the nature of the foods comprising the experimental diets permits to some extent a comparison of the effect

NOTE: Since dairy butter is a common constituent of nearly all diets the following résumé of digestion experiments, conducted by the author while employed as nutrition expert at the U. S. Dept. of Agri., is given to supply information concerning the effect of other food materials on the digestibility of butter.